ROYAL ASTRONOMICAL SOCIETY.

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J. C. ADAMS, Esq., President, in the Chair.

Rev. W. C. Williams, North London Collegiate School; and Richard Lee, Esq., Wheatley, Oxon,

were balloted for and duly elected Fellows of the Society.

DISCOVERY OF THALIA.

On the evening of December 15th, at $6^h\ 30^m$ mean time, Mr. Hind detected, at Mr. Bishop's Observatory, another small planet, belonging to the group between *Mars* and *Jupiter*. Like *Calliope*, this planet has been brought to light by means of the ecliptical star-maps, which are now in course of publication from the above Observatory. The following are the early observations of the planet, which has been named *Thalia* by Mr. Bishop. It shines as a star of between the tenth and eleventh magnitudes, with a pale watery light.

1852.	Green. M.T. h m s	App. R.A. h m s	App. N.P.D. ° ′ ″
Dec. 15	7 18 39	3 12 4·98	73 10 16·4
	8 42 2	2·70	3·9
17	6 2 6	3 10 48·23	73 3 26·0
20	6 17 14	3 9 4·87	72 52 3·9

LUTETIA.

Elements.

By M. George Rümker, from the Observations at Paris, Nov. 18; and at Hamburg, Nov. 28 and Dec. 3.

M	= 61 ° 29 ′ 23·″3	1852, Dec. 0, Berlin M.T.
π	309 53 19·4	1853, Jan. 0, Mean Equinox
Ω	78 38 48·4	
I	3 19 49·7	
φ	19 51 55·2	
Log a	= 0·4157699	log μ = 2·9263517

An ephemeris, from Dec. 6 to Jan. 3, was sent at the same time, which has been communicated to the principal observers.

MASSILIA.

Elements. By M. G. Rümker.

Computed from the Hamburg Observations, Sept. 30, Oct. 26, and Nov. 19.

M	.....	297 0 46·0	Nov. 0·0, 1852, Berlin M.T.
ϖ	.....	94 32 39·7	Mean Eq^{x}.
Ω	.....	207 8 47·6	Jan. 0, 1853.
i	.....	0 40 28·1	
φ	.....	10 3 27·5	
Log a	.....	0·3890493	
Log μ	.....	0·2664326	

These elements represent the middle observation, thus:

Computation − Observation { +0″·4 in Longitude; +0″·0 in Latitude }

An ephemeris based on these elements was sent by M. G. Rümker at the same time: this has been forwarded to our principal observers.

The observations of the solar eclipse of July 28, 1851, which form Part I. of Vol. XXI. of the *Memoirs*, are now published. There are six plates and 120 pages of letterpress. The price to Fellows is 5*s*., and 10*s*. to the public. There is a separate title-page for the purchasers of the part who may not want the volume.

On a New Method of Computing the Perturbations of Planets. By M. Encke, Director of the Royal Observatory of Berlin. Translated into English and Illustrated with Notes by G. B. Airy, Esq., Astronomer Royal. (Appendix to the *Nautical Almanac* for 1856.)

The main object of the author is to supply an easy and accurate method of computing the perturbations of the various small planets revolving between *Mars* and *Jupiter*, the number of which is constantly increasing from fresh discoveries. The method, however, is applicable to any of the other bodies of the planetary system.

Let t denote the time from which it is proposed to compute the action of the disturbing body. By a slight transformation the differential equations of the motion of the disturbed planet are reduced to the following form:—

$$\xi = \iint \left(X + \frac{k^2}{r^{\circ 3}} \left(3 \frac{x^\circ}{r^\circ} \delta r - \xi \right) \right) dt^2$$

$$\eta = \iint \left(Y + \frac{k^2}{r^{\circ 3}} \left(3 \frac{y^\circ}{r^\circ} \delta r - \eta \right) \right) dt^2$$

$$\zeta = \iint \left(Z + \frac{k^2}{r^{\circ 3}} \left(3 \frac{z^\circ}{r^\circ} \delta r - \zeta \right) \right) dt^2,$$

x°, y°, z°, being the elliptical co-ordinates of the disturbed planet, and r° its radius vector, as deducible from the elements of the orbit in which it was revolving at the instant t; X, Y, Z, the resolved values of the disturbing force in the directions of corresponding rectangular axes; ξ, η, ζ, the perturbations of x, y, z, counted from t; δr, the perturbation of the radius vector; and k a constant quantity.

These equations are integrated by mechanical quadratures, the successive numerical values of the quantity under the sign of the double integral in each equation being corrected by a peculiar process, founded on the theory of finite differences.

The first step is to divide the period for which the perturbations are to be computed, into a number of sufficiently small intervals of time, each equal to τ, and to calculate the values of X, Y, Z, for the times $t - \frac{1}{2}\tau$, $t + \frac{1}{2}\tau$, $t + \frac{3}{2}\tau$, &c., using for this purpose the elliptical co-ordinates x°, y°, z°, of the disturbed planet. As ξ, η, ζ, $\frac{d\xi}{dt}$, $\frac{d\eta}{dt}$, $\frac{d\zeta}{dt}$, are assumed severally equal to zero at the commencement of the period for which the perturbations are to be computed, the first three values of the quantities under the sign of the double integral (or $\frac{d^2\xi}{dt^2}$, $\frac{d^2\eta}{dt^2}$, $\frac{d^2\zeta}{dt^2}$), are readily found with sufficient accuracy by an approximative process, and from them the corresponding values of ξ, η, ζ, are deduced. For all the subsequent values of ξ, η, ζ, the computation is uniform. The mode of procedure is this:—From the three last values of $\frac{d^2\xi}{dt^2}$ an approximate value of the same quantity for the following argument is readily deduced, and a provisional value of ξ is then found by means of the formula of integration $\xi = ''f + \frac{1}{12} \cdot \frac{d^2\xi}{dt^2}$, $''f$ being a numerical quantity computed with the utmost facility by the aid of results already obtained. The corresponding values of η and ζ being found by a similar process, the three resulting numbers are employed in computing the second part of the expression under the integral sign in each of the fundamental equations. Finally, the numbers thus found, when applied to X, Y, Z, give the corrected values of $\frac{d^2\xi}{dt^2}$, $\frac{d^2\eta}{dt^2}$, $\frac{d^2\zeta}{dt^2}$, which in their turn lead by double summation to the true values of ξ, η, ζ. By a similar process the values of ξ, η, ζ, for the next argument are ascertained, and so on in succession, the operation being rapidly carried on throughout the entire period for which the perturbations are to be computed.

In order to test the practical utility of this method, M. Encke compared its results with those obtained by the method of the variation of arbitrary constants. For this purpose the perturbations of *Vesta* by *Jupiter* from 1854, May 24, to 1855, October 7, were subjected to a double computation. The following table exhibits the values of the polar co-ordinates of the disturbed planet at the end of this period, as resulting from the use of both methods:—

	Variation of Constants.	New Method.
	° ′ ″	° ′ ″
Longitude	335 20 6·4	335 20 6·6
Latitude	−5 40 42·3	−5 40 42·3
Log. Rad. Vector	0·3665032	0·3665042

M. Encke mentions as a proof of the easy application of this method, that in computing the perturbations of *Vesta* with given places of *Jupiter* for thirteen epochs, only eleven hours were occupied, notwithstanding that he was frequently interrupted in the course of the operation.

If extreme accuracy is required, the values of x, y, z, resulting from the application of ξ, η, ζ, to x°, y°, z°, may be employed in computing X, Y, Z, and then by repeating the operation we may obtain corrected values of ξ, η, ζ. This process, which gives the perturbations depending on the square of the disturbing force, may be repeated over and over again until the assumed and resulting values of ξ, η, ζ, finally agree. The method, however, is so rapidly exhaustive that this object is generally secured at the first repetition.

When the perturbations are large the method is still practicable, but it becomes necessary in this case to reduce in a corresponding degree the magnitude of the intervals of quadrature.

The following are some of the principal advantages attending this method of computation: —

1. It proceeds from the fundamental equations of motion by a series of arithmetical operations of extreme simplicity, only a few analytical expressions of by no means a complicated character being employed in the computation.

2. It affords the means of immediately correcting the quantities employed in computing the disturbing forces for each successive interval, and thereby leads rapidly to results of great accuracy.

3. Since the perturbations of x, y, z, are of the same order as those of the polar co-ordinates, they serve to afford unequivocal indication of the extent to which the intervals of quadrature ought to be diminished. It is well known that the method of the variation of constants may assign enormous perturbations to the elements while at the same time the actual perturbations may be very insignificant.

The original researches of M. Encke on this subject are contained in Nos. 791, 792, and 814 of the *Astronomische Nachrichten*. It may be mentioned that a similar method had been already proposed by Mr. G. P. Bond, of Cambridge, U.S. (*Memoirs of the American Academy of Arts and Sciences*, vol. iv. part i. p. 189). M. Encke, however, did not become acquainted with this circumstance until he had completed his investigation.

Extract of a Letter from Mr. Nasmyth on the Red Prominences seen in Total Eclipses of the Sun.

"In reference to the note at page 210, in the last *Monthly Notice*, touching the proposed mode of enabling us to get a *chance* of a peep of the sun's red flames any clear day about midsummer when he is near the meridian; I believe I may claim to be 'the party' who first broached the notion of the dark chamber and the white screen with the hole in it, so as to let the sun's image pour into a black box, and so get rid of his light on the screen, while the image of the red flames might have a chance to be left behind on the screen. I first turned my attention to this subject on reading the accounts of the last total eclipse, July 28, 1851, regretting that so very limited a time was allowed for making any deliberate observations on that occasion. If my plan could be got to succeed, I doubt not it would tend to supply us with such facts as would enable us to get more insight into the nature of the red flame phenomena; otherwise, I fear, it may require ages to afford the requisite opportunities.

"I communicated my plan with sketches to the Astronomer Royal, and as he appeared to enter warmly into the subject, and proposed to try the experiment, I sent him some brass-work for the mounting of the telescope he proposed to devote to the purpose; but I suppose he has been so occupied with more important affairs, that I have not had the pleasure to hear from him whether he has had any result."

[Here Mr. Nasmyth gives the sketch of an arrangement well adapted to the purpose.]

"If no one having the desired means and leisure will make the attack, I shall hope to do so myself some fine midsummer day.

"I sent a sketch to Mr. Hind and Mr. Lassell. I think it was about August 1851 I did so. Pray pardon me troubling you on this subject; but as the matter has been alluded to in the *Notices*, perhaps you may think fit to name the subject in the next. I also proposed it to Professor Piazzi Smyth, who made some experiments, and saw some remarkable pink or rose-coloured appearances; but Mr. Airy is under the impression that these were due to some instrumental agency.

"I am working away at the moon's surface most steadily and carefully; and hope to leave that subject advanced a step or two ere I am called away. My 20 aperture reflector is a noble tool for this work; and I have now a very respectable figure which, weather permitting, yields me glorious treats in rummaging among the banks and braes of the lunar craters. I have with 450 as much light as the eye can well support for a spell of a couple of hours' observation and sketching; so, were the instrument of greater aperture, it would be of no service, in proportion, for this my *work:* for I have determined to devote myself to it, and not leave off till I have left a few marks of progress."

Extract of a Letter from Professor Secchi to Mr. Grant, F.R.A.S.

"*Rome, May* 8, 1852.

"Allow me, dear Sir, to present you my most sincere thanks for the kind letter you addressed to me, and for the valuable present of your book, which was joined with it. I am very far from performing things worthy of the historical renown of our country; but I will try to make the best use of those means which are put at my disposition, although they are, indeed, very scanty: if in other times they were at the level of science, they are now decidedly below it. I hope, however, this state will not last long, and that some substantial improvement will be made to our observatory. The history which you have written is that of the most attractive part of the science, and which was wanted not only in England, but also in Italy, and I have no doubt it will be well received here. I would have written to you sooner, but I waited a little while, to be able to inform you of some researches which I have undertaken on the distribution of heat at the surface of the sun, and on the constitution of the lunar geology, which I hope will be well received by you.

"As to the first subject, you know that it is generally admitted that light is brighter at the centre of the solar disc than at the edges. M. Fizeau proved that the same diminution subsisted for chemical radiation, and I obtained a confirmation of it in the daguerreotypes of the sun taken during the last solar eclipse. But I had no proof that the fact would be the same with heat, although this was highly probable. I tried, therefore, to fill this blank, and have instituted a series of researches on the temperature of the different parts of the solar disc. I have been thus led to some interesting conclusions quite new to me; but before exposing them, I think it necessary to say something on the method of conducting the experiments. A thermo-electric pile, belonging to a very nice Melloni's apparatus, was attached to the telescope of our equatoreal, and fixed on the prolongation of its optical axis at a distance of about 15^{cm} from the eye-piece: the image of the sun, magnified by the eye-piece itself, was thus projected on the pile, and had a diameter of 12^{cm} nearly. When a leaf of white paper was put in the place of the pile, the image was perfectly defined, and the spots neatly terminated. By a diaphragm, the aperture of which was 4^{mm} broad, and 15^{mm} long, disposed parallel to the edge of the disc, a small portion of the sun's image, which was about $1'$ of arc broad, and $4'$ long, was allowed to radiate on the pile. The construction of the equatoreal enabled me to keep the same points of the disc steadily fixed on the pile, as far as it was necessary to obtain an invariable deflection of the needle of the galvanometer, and the position of the points of the sun, under examination, was readily deduced from the reading of the declination circle. There is no need to observe that all extraneous radiation was pre-

vented by large screens, supported by the telescope itself, and the turning dome was only opened as far as it was necessary to introduce solar rays on the object-glass. The telescope has $1^{m}{\cdot}25$ focal length, and 77^{mm} aperture.

" Now these are the results obtained in sundry experiments: 1. The heat of the solar image is at the centre almost twice as great as at the borders. This is found to be true, examining the diameters both in right ascension and declination; 2. The maximum of temperature did not appear to be at the centre, but above it, in a point distant from it about 3′ of geocentric declination. Constructing graphically the curve of the intensity of heat, taking as *abscissæ* the parts of the sun's diameter, and as *ordinatæ* the intensities themselves, it appears that this curve (a kind of inverted parabola) is not symmetrically disposed about the axis of the ordinates, but a good deal inclined towards the upper edge. I subjoin some numbers which represent the intensity of heat in the parts of the diameter of the sun taken in minutes, + above and — below the centre of the image.

Positions on the diameter of the sun in declination	+14′·96	+11′·32	+ 3′·00	+ 1′·32	−10′·9	−14′·88
Relative intensity of heat	57·39	88·81	100·00	99·48	81·32	54·34

These are the results of eight series of experiments, none of which is found in contradiction with the others, and their separate numbers are very nearly the same, so that the fact seems to me completely ascertained. It is certainly curious that the maximum of heat corresponds with the position of the solar equator, as visible from the earth at the epoch of the experiment (20th, 21st, 22d March). This leads naturally to the conclusion that the solar equatoreal regions must be hotter than the polar regions, as was suspected already from the more frequent appearance of the spots there. The conclusion seems perfectly accurate, even admitting a solar atmosphere, since the effect of this last should be to diminish symmetrically the radiation around the centre of the image; on the contrary, if the polar regions are less hot than the equatoreal, the intensity of heat should have been less in the lower part of the image, where the south pole of the sun was visible; and, consequently, the parts having equal distance from the centre of the image had a very different heliographical latitude, on account of the inclination of the solar axis to the ecliptic. From these principles only, the non-symmetry of the curve is accounted for. If *this alone* is the cause, the curve will be found symmetrical in the months of June and December, and reversed in September, since in the two former the equator passes through the centre of the image, and in the last is below it. But it is not impossible that the two solar hemispheres should possess different temperatures, as seems to be the case on the earth, and is suspected in *Mars*. If this is the case, these researches will throw some light on the climatology of the earth itself; since the heat of the sun must be different, according as one or the other of its poles is turned towards the

earth. Future experiments will resolve this question. With respect to the poles of the sun, I shall add here a conjecture on a fact recently discovered by Captain Sabine. The journal *Institut* relates that this gentleman has found that the deviation of the magnet from its mean position at the Cape of Good Hope is found to be in opposite directions at the epochs of the two equinoxes. Might this not be an effect of the solar magnetical polarity on the terrestrial magnetism? The fact deserves to be examined, if it takes place in our hemisphere, and in opposite directions. Coming again to the solar heat, I have found that spots seemed less hot than the rest; but as only small groups of them were visible, no singular fact or law can be stated from these observations. I shall conclude this account by noticing an odd historical coincidence, namely, that these observations were made in the same room where it is said F. Scheiner, the first who used a telescope mounted equatorially, made his observations of the sun. This room has been this year added to the observatory.

"I shall now briefly expose to you some considerations on the theory of lunar formations, which have been the subject of some discussions with our eminent geologist, Professor Ponzi, whom I gladly engaged in these researches. He is exceedingly well acquainted with the terrestrial volcanoes, both ancient and modern, (being now occupied in the description of the Roman States), and with Italian geology: the conclusions to which we have arrived are these.—The lunar volcanic formations must be distributed into three classes, to which a fourth class may be added analogous to our Plutonian formations.

"The first class of the lunar volcanoes possesses a distinctive character; that the edges of the craters are almost completely obliterated, so that their border now is a continuation of the plane ground, in which they seem excavated, and a deep well only remains in the place of the ancient mouth of the volcano. Instances of this kind are very frequent near the south pole of the moon, and around the large spot Tycho; but Tycho itself does not belong to this class. The physiognomy of these craters nearly resembles our submarine volcanoes of the Monti Ciminii to the N.W. of Rome. The country around the craters of Bracciano, Bolsena, di Vico, is almost flat, and the old openings of the craters are now deep lakes. On this ground we are led to believe that even in the moon many subaqueous volcanoes existed. Another distinct character of these volcanoes of the first class is, that they are in a line, as if they burst from the cracks of the solid body of the crust produced by earlier formations: this is most striking in Arzahel, Purbach, Alphonsus, and many others, and they seem to follow the cracks made by the *soulèvement* which raised Tycho, the lunar Appennines, &c. Some of the higher chains of lunar mountains are seen visibly parallel to the alignement of the craters: this fact also is like that which we observe on the earth; indeed, the large Italian volcanic chain follows the line of the Apennines along this country.

" The second class of lunar volcanoes are those which have their outside edges elevated above the surrounding plane; their form is generally regular, and not broken, as those of the preceding class, and the ground around them is elevated in a radiating disposition, as is visible around Tycho, Copernicus, Aristotle, &c. The regularity of their forms suggests that the ejected matter was not disturbed by the motion of waves, and, consequently, that they were atmospherical volcanoes, like those of the Monti Laziali, Albani, and Tusculani, at the south-east of Rome; the want of breach in the craters seems to indicate that no lava, but only scoriæ and loose matters have been ejected. The disposition of the soil around them suggests the opinion that they are of a comparatively later epoch, and formed after the crust of the satellite was pretty resistent, and was capable of being elevated all round by a great effort. It is singular, indeed, that this *radiation* of the soil around is found proportional to the magnitude of the central crater. The effect of this *soulèvement* extended sometimes to a prodigious distance, comparable to that of the Cordilleras of the Andes on the earth. The greater part of the craters of both the classes now described possesses an insulated rock inside, very seldom appearing (at least in commonly good telescopes) perforated. This bears great analogy with what we see in more than one place in the ancient volcanoes of the earth, where the erupting mouth has been stopped by a dome of trachytic matter as by a stump. Monte Venere, near Rome, is of this formation, and lies in the centre of an immense old crater.

" The third class of lunar craters is very small, and bears a great likeness with those called by geologists adventitious craters, and seems to be of a very late formation, the last efforts of the expiring volcanic force. They are irregularly scattered through all the moon, but occur more frequently at the borders or inside of the old demolished craters, although not concentric with them, and seem to have been produced after the large ones were completely closed, either by trachytic ejection or by becoming lakes. These small craters have very seldom rocks inside, or a flat bottom; but their cavity is conical, and does not exceed in dimension our common volcanoes, which are yet active on the earth. From these facts and observations it appears, that volcanic action has gone on in the moon through all the same stages which it has gone and is going on in the earth, and is there, probably, completely extinguished, on account of the smaller mass of the moon, which has been cooled very rapidly. This rapidity of cooling, joined with the smaller gravity, may account for the great developement of volcanism there, and comparatively fewer Plutonian formations. But extensive instances of this kind are not wanting; the lunar Alps, the Apennines, the Ripheæ, &c. may represent this formation, surrounding vast basins, and having modern volcanoes following the direction of the higher edges of their chains. Professor Ponzi seems to think it unquestionable that water existed at the surface of the moon; the fierce glare of the sunshine is not able to melt the ice there, which is, probably, at the temperature of the planetary spaces; just

as the sun at the surface of the earth is not able to melt our glaciers, which yet possess a certainly higher temperature. Cold, and other unknown causes, may have absorbed and fixed all the atmosphere which anciently existed, as we see that the immense atmosphere which anciently surrounded the earth has been fixed by several chemical processes and reduced to its actual composition; and it might be possible that this actually existing atmosphere of ours should be all solidified, either by cold or chemical processes, if the earth arrives at the same degree of cold which seems to have place on the moon.

"These are some of the conclusions to which we have arrived in our inquiry into the solar and lunar constitution; they do not certainly belong to astronomy strictly taken, but, a recreation from more serious occupations may be excused to astronomers dwelling in the pure climate of Italy. I trust you will receive them as a token of my gratitude, and of interest for the work which you have published."

In a later letter Professor Secchi tells Captain Manners "that the observatory of the Collegio Romano is about to be furnished with a large refractor equatoreally mounted. The refractor will have 9 inches aperture, and a focal length of 14 French feet." There is only a little hesitation at present as to the best mode of mounting this telescope.

Observations Héliothermiques, par Sig. Secchi.

(*Comptes Rendus, Oct.* 26, 1852.)

Since the date of the preceding communication, Sig. Secchi has made the following interesting experiments on the heat emitted by different parts of the solar disc. It appeared from the former experiments that the heat increased from the margin of the solar disc to the centre; but there was this peculiarity, that when it was measured in the direction of the axis of the sun's rotation, it attained its maximum intensity at a point situate about 3′ above the centre. At the time when the former experiments were made, the projection of the sun's equator upon his disc passed above his centre, at a distance agreeing almost exactly with the position of maximum temperature; whence the conclusion naturally suggested itself to Sig. Secchi, that the equatoreal regions of the sun are intrinsically hotter than the other parts of his surface. He justly remarked, that if this were true, it ought to follow that in the month of June, when the nodes of the solar equator pass through the earth, the upper and lower hemispheres should appear equally heated; and on the other hand, in the month of September, when the projection of the solar equator attained its greatest depression, the point of maximum temperature should fall *below* the centre of the disc. Experiments similar to those previously undertaken by him have served to confirm this view of the subject. "In the month of August the upper hemisphere appeared a little hotter than the

lower, but the differences, always very small, were not unfrequently negative. In September *the lower hemisphere appeared decidedly the hotter of the two.* The maximum difference rather exceeded 2° (by the galvanometer), being almost identical with the result obtained in the month of March *in the opposite direction.* Moreover, this maximum occurred from the 14th to the 16th September, which also corresponds with the epoch of the greatest depression of the solar equator."

Sig. Secchi remarks, that the influence of the solar spots upon the temperature is very striking. Sometimes a spot, which did not occupy more than $\frac{1}{100}$th of the aperture of the pile, caused the temperature to fall 3°, or about $\frac{1}{15}$th of the whole intensity. He found, however, by isolating the spot with a diaphragm, that it exercises a sensible influence even in the obscure part.

Extract of a Letter from Mr. Lassell.

"Valetta, 1st Nov. 1852.

"You will, I hope, have received a letter I wrote to you shortly after my arrival, giving you my first naked-eye impressions of this climate. I am now enabled to say that I find it quite as favourable for the use of the telescope as I had anticipated — incomparably more so than at Starfield, both in respect of quantity and quality of clear sky. My first observation through the telescope was on the 5th of last month, but it was not completed in respect of clock-motion and driving apparatus until the 16th, and on the 26th we were visited with a tremendous "*gregale*," which, beside causing some shipwrecks with loss of life in the harbour, and upsetting a whole battery of 24-pounders as if they had been ninepins, gave my roof a tremendous shake, and, partly dislodging it from the wall, would, I believe, have carried it quite away, if it had not "brought up" against the strong frame-work of the polar axis. Happily no injury was done to the telescope beyond crushing the tube of the finder, and that without injury to the optical part. This has lost me some eight or ten days; but all the mischief is now repaired, and since the 27th I have observed every night, the weather having settled again so as to be even finer than before the storm. The principal objects viewed are, as you may surmise, *Neptune*, *Uranus*, and *Saturn*, the last being an object of such surpassing interest as to absorb almost more than its share of attention. I send you herewith a careful drawing of its present appearance; and, if viewed through a blackened tube with one eye, at the distance of three or four feet, it presents so exact a fac-simile of the picture afforded by the 20-foot telescope and a power of 650 that I know not how to introduce a correction. I have compared it carefully with the telescopic image, by having it well illuminated and placed alongside the eye-piece, in order to form this estimation, and the only thing that suggests itself as a possible improvement of its accuracy, would be *a very slight* increase of breadth of the outer

ring at the expense of the breadth of inner bright ring. To rival the original, however, in *beauty*, I find to be impossible, especially from want of proper colouring. The two principal equatoreal belts are of a ruddy brown colour, changing pretty suddenly at the southern edge of the most southern of these belts into a dusky bluish-green colour, very much deepened around the south pole, the exact place of which seems to be marked out by a circular lighter shade. This dusky portion is much variegated by minute dark stripes and lines, which defy imitation with the pencil. One of the most striking attendant phenomena which I now note for the first time, is the *evident transparency of the obscure ring*. [I intend in future, in speaking of the rings, to adopt M. Otto Struve's nomenclature, calling the outer ring *A*, the intermediate *B*, and the obscure ring *C*.] The ring C, crossing the ball, is of a much lighter texture or colour than the other parts, and both limbs of the planet can readily be traced through it, and quite as plainly as in the drawing, when the latter is treated as I have pointed out. The effect is precisely that of a band of *crape* stretched within the ring, which, projected on the white ball, would appear of a lighter hue than where projected on the dark sky. There is evidently, also, a sudden paleness of the main division of the rings where it crosses the ball—scarcely sufficiently marked in the drawing—as if in some degree, at least, the ball were seen through the division. The shading on the inner edge of the ring *B* extends clearly within one-third of its entire breadth of the outer edge, and during some rare glimpses appeared to me as if it were constructed in narrow concentric circles. No division, properly so called, could at any time be traced or imagined on the ring *A*; but there was an evident *shade* in the middle of its breadth, and occupying about one-third of it. This was seen night after night with different powers, from 297 to 650, so that I am perfectly satisfied of its existence, nearly as shown in the drawing. Its depth of shade is rather deeper than the darkest shading of *B* close to its inner edge.

"A remarkable sudden projection of the shadow of the ball on the ring, where it crosses the division, was noticed especially from the 5th to the middle of October, when the shadow was much larger than it now is. It is just indicated in the drawing, but not so palpably as in the original when best seen. I am not able to say whether or not it is an optical deception, arising from the juxtaposition of the division and shadow. A more remarkable phenomenon was repeatedly noticed on the opposite limb, appearing like an elongated spot, sometimes extending up to the division, but generally falling short of it, and projected on the ring *B*. That something was there I am persuaded, but I can offer no explanation of it.

"I must now allude to a most extraordinary anomaly, which I confess myself utterly at a loss to explain. Notwithstanding the exquisite views I have had of the planet since my arrival here, far exceeding in sharpness of definition all I have ever seen at Starfield, I have never obtained a single glimpse of the division in the ring *C*,

seen and measured micrometrically by M. Otto Struve; nor of the ring being concentrally divided into two, as seen by Mr. Dawes. On every occasion, and when most sharply defined, it appears of one uniform texture and depth of shape, constantly conveying to my mind the idea of network or a crape veil, as I originally described it. Moreover, when best seen, the part crossing the ball seemed of uniform density, the northern edge not appearing sensibly darker than the southern.

"If what I have described should not give an adequate notion of the amount of precision or sharpness of the telescopic image, I may add, that *Enceladus*, even when over the ball, and now not more than the narrowest breadth of the ring from it, is not to be overlooked; and *Mimas*, when at a considerable distance from his greatest elongation, is steadily seen with various powers, from 297 to 650, with the splendour of the planet fully view. The vast difference of visibility of *Mimas* and *Enceladus* is only to be appreciated by those who have seen both together.

"The superiority of vision I am now enjoying is not confined, however, to *Saturn;* I am enabled to watch the satellite of *Neptune* every night — completely round his orbit. On the 30th of October I observed him very near his closest approach to the planet, and certainly then with extreme difficulty, being, as nearly as I could judge, not more than eighth-tenths of the planet's diameter from his limb. My usual micrometer powers, of 614 and 760, failed to show the satellite, and it was only after trying a succession of single lenses that a power of 1917 at last brought it out. The two new satellites of *Uranus*, too, *Ariel* and *Umbriel*, I have been enabled, not only to see, but to measure micrometrically—a thing I was never able to do in England, as I was there always obliged to hide the planet in order to see them. I hope, therefore, to get a large and good series of measures of these and of the satellite of *Neptune*, as well as of the old satellites of *Uranus*, the results of which I will send you in due time.

"I believe, however, that *all* the advantages of vision of which I speak do not arise from the climate and greater altitude of the objects, but in some sensible degree from the increased perfection of the figure of the speculum. You will remember I some time ago mentioned that I was engaged in some attempts to improve the action of the polishing-machine, in producing the utmost attainable *regularity* of curve. After a great many elaborate contrivances, none of which satisfied me, I hit upon a very simple modification, which, I think, answers the end perfectly, and enables me to produce far more perfect figures, and with much more ease and certainty than formerly. I intend to take an early opportunity of sending you a description of the mechanism superadded to the machine. After the addition of this improvement, I refigured, shortly before I left home, specula *A* and *C*, both of which I brought with me, but as yet I have only worked with *A*.

"I presume that, unless another hurricane blows away my observatory, I am located here for the next three or four months."

Extract of a Letter from Mr. Lassell to Mr. W. De la Rue.

"*Valetta, 8th Nov.* 1852.

"My dear Sir,— I am ashamed of having so long delayed to thank you for the 'skeleton forms' of *Saturn* you have been so kind as to send me. How did you draw the outline so perfectly? It seems to me the best representation of a circle in perspective I have ever seen.

"I sent a drawing of the planet to the Astronomical Society, which may possibly be on the table at the meeting of this month, exhibiting such phenomena as I had at that time been able to make out, using your 'form' as a scale, and making such changes in the proportions of the ball and rings as I thought necessary. Your drawing, however, I think generally so correct, that the variations I have introduced are but small. As I sent a pretty long letter to Mr. Sheepshanks, detailing what I had then seen, it is unnecessary to repeat what you will already have heard; I may add that I was then able to state the result of only a very few observations, and those made under the disadvantages of an unfinished observatory. I am happy to tell you, that both in respect of the skies here and of the performance of the telescope, I have reason for the highest satisfaction. Since the restoration of the Observatory, on the 27th ult., after the storm, there has not been a single cloudy night, nor one unfit even for delicate work; so that you may suppose that observing, recording, and taking needful repose, leave me scarce a moment's leisure. The nights are as remarkable for tranquillity as transparency; and I have not encountered one of those nights so frequent at Starfield, on which I only opened the Observatory to close it in disgust.

"The satellite of *Neptune* is a bright object, not to be for a moment overlooked by the most cursory observer, and the two new satellites of *Uranus* are constantly conspicuous in the field of an eye-piece magnifying 1018 times, which is, indeed, the best power, in the finer states of the air, for exhibiting both planet and satellites. I am enabled also to add that the figure of the speculum appears to be all that could be wished or desired. It is the first fruits of the transverse motion superadded to the polishing machine, rendering it in my judgment a perfect engine for figuring specula. The night of the 4th inst. when the air was somewhat better than usual, *Saturn* was exhibited in a style that beggars description; power 650. The number of minute belts and stripes all over the ball defies my delineating powers; but I shall try to record the main phenomena.

P.S. "The night of the 4th *confirmed* all I had previously seen, but scarcely *added* anything more, except revealing more minute and numerous markings on the ball."

Letter from Mr. Lassell to Mr. Sheepshanks.

"*Valetta, Nov.* 25, 1852.

"Since my letter to you of the 2d, I have been favoured with one or two nights of finer quality than usual, especially the 4th inst., when the planet *Uranus* and his four satellites were exhibited with a power of 1018 in a style I never saw before or ever hoped to see. The near satellites were bright objects, and not for a moment to be overlooked. The disc of the planet was harder and sharper than I have ever seen it with any power in England, yet I could not satisfactorily make out any phenomena upon its surface. Under these favourable circumstances I carefully surveyed the planet round to the distance of $2'\frac{1}{2}$, without finding any points of light which might probably be other satellites. I obtained on this occasion good micrometrical measures in position and distance of all the four satellites, and also some measures of the diameter of the disc.

"The view of *Saturn* with 650 left nothing to desire in hardness and sharpness of definition, except the removal of some slight atmospheric disturbances rendered visible with this power. So far as the telescope was concerned, no defect whatever of the image was appreciable. The view with 22 inches was compared with that afforded by the full aperture, and the latter was preferred, the outside annulus appearing to add to the light and sharpness of the image, though it diminished the blackness of the field. A number of minute and exquisite *markings* on the southern portion of the ball, together with three distinct dark blue or bluish-green belts, were for the first time distinctly noted; and both limbs of the planet were most distinctly traced *through the obscure ring*, quite up to the inner edge of the inner bright ring. What can this marvellous and unique appendage—the obscure ring—be? I do not venture much into the field of speculation myself, but I can scarcely help wondering whether this sort of tangible nebula can be a connecting link between the more attenuated nebulæ of the universe, and the solid bodies of our planetary system?

"In other respects, the views of *Saturn* obtained on this remarkably fine night did not add much to the phenomena I had previously seen by glimpses; but my attention became more directed to the delineation of the belts and phenomena of the ball; and between this night and the 13th inst., I made a number of observations, which by means of description, and a sufficiently rude drawing sent herewith, I will attempt to put upon record. First, then, referring to Mr. De la Rue's diagram, and reckoning southward from the southern portion of the obscure ring where it crosses the ball, there is the brilliant zone about equal in breadth to the sum of the two bright rings at their narrowest part. Secondly, the principal ruddy belt extending from about 7° to 28° south latitude. Thirdly, a white zone about 5° broad, not, however, so brilliant as the first zone, nor so uniform and well defined in its brightness.

Fourthly, a yellowish ruddy belt terminating sharply about 45°, in. Fifthly, an extremely narrow dark blue or bluish-green belt, Sixthly, another light zone, but of a deeper shade, and marking the commencement of the bluish-green portion. Seventhly, a lighter narrow blue belt passing through the 60th degree. Eighthly, a still deeper-shaded light zone. Ninthly, a sharply-defined, very dark blue belt, a little above the 70th degree, and encircling the pole. Of this belt about three-fourths of the whole circle can be seen, including within it the pole itself, marked by a lighter-coloured spot of about 20° diameter. Thus, if the less shaded parts, or those I have called zones, be the body of the planet, there will be distinctly traced *five belts*,—two ruddy and three of a neutral tint or bluish-green colour. I scarcely know how to apologise sufficiently for the rudeness of the drawing; but not being an artist, I have not attempted to produce an artistic effect, but only to convey a correct idea. And I have, I believe, only further to observe, that while I have not attempted to introduce the more delicate lines, something like what I have drawn undoubtedly exists on the planet, though softened down and made infinitely less *staring* than it is in the drawing.

"The ambiguous 'spot' on the south-following portion of ring *B*, close to the ball mentioned in my former letter, appears to have been the germ of the coming shadow, which, on the 15th inst. had become plain on the following portion of ring *B*, without being visible on ring *A*, and while the shadow still lingered on the preceding side. This lingering shadow had now assumed the form which the 'spot' previously spoken of presented in the early part of October, and would appear to be the 'remarkable sudden projection of the shadow of the ball' there spoken of. Whether or no this projection indicates, as has been suggested, some peculiarity of form of the edges of the rings, I certainly saw the shadow itself fairly entered on ring *B* on the following side, while it still remained visible as a fine line on the preceding side.

"I shall obtain, I doubt not, some additional testimony about the orbit of *Hyperion*, having watched him through an entire period, and obtained measures of him even at both his opposition and conjunction.

"This is a marvellous climate, a single month yielding almost three times as many observations of the satellites of *Uranus* and *Neptune* as I got all last year! The last three days, however, the weather has broken, and we have violent gusts of wind and tremendous showers of rain."

On the Appearances of Saturn. By Mr. Dawes. With a Drawing.

"The accompanying sketch of the planet *Saturn* represents the principal phenomena seen by me during the month immediately subsequent to the 19th of last September. One of Mr. Warren

De la Rue's lithographed outlines (with some of which he kindly supplied me) has been employed, as facilitating comparison with sketches by other observers, by whom the same outline may be used. I have, however, in this sketch considerably reduced the breadth of the projection of the obscure ring on the ball, as it was originally much broader than it appeared in my telescope.

"The autumn has hitherto presented few favourable opportunities for the use of high telescopic magnifiers; but the nights of September 20th and 25th were very fine, especially the latter. Some of the notes entered on those nights I will here transcribe from my journal.

"'1852, Sept. 20. $8\frac{1}{2}$-foot achromatic. The faint ring at times comes out well. There appears to me to be a *dark line* between it and the bright ring. Power 366.

"'The inner edge of the inner bright ring has, as I observed last year, a very narrow line, considerably brighter than the portion immediately exterior to it: in other words, the dark shading of the inner ring towards its interior edge does not extend quite to the edge; or, at least, it is not so dark at the edge' (illustrated by a diagram in journal). 'The dark shading has a very defined appearance, like a belt with rather sharp edges.

"'The two belts on the ball just south of the equator are pretty well defined; but the most southern is not much darker than the cap-like belt which covers the south pole.'

"'Sept. 25. $8\frac{1}{2}$-foot achromatic. *Saturn;* power 460; single convex lens. Extremely distinct when the fog is not too thick to afford sufficient light. A very narrow dark line on the outer bright ring; the portion 'of the ring' exterior to this line is *less luminous* than the portion interior to it. The brightest part of the outer ring is that close to the division between it and the inner bright ring.

"'The shading on the inner bright ring is *streaky;* and it extends towards the exterior edge of the ring to within about one-fifth of its whole breadth. A dark and distinct shading, like a sharply-defined belt, is seen *near* the inner edge, but the edge itself is obviously brighter, though the brighter portion is extremely narrow.

"'The dark ring is visible, but is very faint, owing to the fog; and occasionally its exterior portion only is seen.

"'The projection of the dark ring on the ball is by no means *black;* neither is the southern edge of the dark line much, if at all, blacker than the rest of it. It appears as though the *shadow* of the faint ring were not really *black*, rendering it probable that the faint ring may be *partially transparent*. There is a striking contrast between the dark gray projection of this ring and the shadow of the ball on the bright ring, which is perfectly black.'

"This last paragraph gives the result of my endeavours to discern a difference of colour between the *projection* of the faint ring on the ball and the *shadow* which it must at that time have cast *beyond its own inner edge;* and which I expected might possibly be perceived as a *black* line distinguishable from the *gray* projection. But though the whole breadth of the dark line, made up of the

faint ring and its shadow, was obviously broader than could belong to the projection alone, no difference could be perceived in the degree of darkness.

"On three fine nights in October, being the 18th, 19th, and 20th, I noticed a well-defined narrow belt at about 40° or 45° of south latitude on the planet. This I did not observe last year. The division of the broad belt (just south of the equator) throughout its whole length by a narrow bright line is more distinct and uniform than it usually was last year. Towards the close of last year's apparition I noticed that it had become more distinct in some portions of the planet's circumference than in others.

"The belt-like cap which covers the whole of the southern hemisphere is considerably less dark near the south pole, agreeing in this respect with the appearance last year. But when best seen, the greater part of the hemisphere appears streaky in the direction parallel to the equator.

"On the above-mentioned fine nights in October, my friend the Rev. C. Strong, who was observing with me, frequently had his attention arrested, while looking at other objects (the faint satellites especially), by the dark line on the outer ring; and remarked that it gave the impression of a division of the ring at a point a little exterior to the middle of its breadth. It is certainly remarkable that this line has continued to be visible in my telescope under fine circumstances for the last two years; while the far larger telescopes of the Harvard and Pulkova observatories, and the Starfield 20-foot reflector, have all shown the ring without even a variation of shade throughout its breadth. Is it not possible that in telescopic, as well as in microscopic observations, a certain quantity of light may be the most favourable for the discerning of extremely narrow dark lines on a luminous ground? Certainly, in my own telescope, such delicate phenomena are not most plainly seen when the planet is most brilliant; but rather, as on the exquisite night of Oct. 26, 1851, when the heavenly bodies are shorn of much of their usual splendour.

"*Wateringbury, November* 1852."

"Since the date of the observations which I communicated to the Society in the beginning of November, few opportunities have occurred of making further acquaintance with the phenomena of *Saturn*. On Nov. 16th, the planet was seen bright and clear between clouds for a very short time, during which the semi-transparency of the obscure ring was distinctly recognised with my refractor, power 323. I had that morning received from Mr. Lassell (under date 'Malta,' Nov. 9), a long and interesting account of his discovery of this fact; but expected that it would probably require the finest possible circumstances to render it distinctly visible in my telescope. I was, therefore, agreeably surprised to find it by no means very difficult to make out in a

moderately good state of the air; and on the 18th, and especially on the 30th, it was seen satisfactorily. The edges of the ball were distinctly traceable down to the inner edge of the bright ring; the projection of the obscure ring having much the appearance of a very dark and distinctly defined belt. Such it was, doubtless, formerly supposed to be; and it is so delineated in Professor Encke's pictures of *Saturn* in the *Transactions of the Berlin Academy for* 1838.

"The dark shading which has for some time surrounded the southern pole appears to me to have formed itself into a more distinct belt than was visible in September. I believe the whole, or very nearly the whole, of this belt is seen, the middle of its breadth being about 12° from the pole, which is comparatively light. On November 30th, the whole region within 30° or 40° of the pole was noticed to have a light greenish blue tint, the tendency to green being more decided than I had before observed. The night was at times very brilliant, and *Enceladus* was distinctly seen.

"The observed semitransparency of the obscure ring has greatly strengthened an idea I have long entertained, founded on its situation and small reflective power, that it may probably be composed of water or some similar fluid; and if this notion be correct, we may reasonably imagine that its condition may be very variable; and, especially, that divisions seen in it at one time may not be recognisable at another. In estimating the degree of its transparency, it must not be overlooked that the light on the portion of the ball, which we see through it, must have passed through its substance *previously* to its reaching the planet, and thus has been *twice* transmitted through it before it comes to us.

"*Wateringbury, December* 1852."

Letter from Rev. W. R. Dawes to Mr. De La Rue.

"Saturday evening was brilliant here, but by no means sufficiently favourable for delicate observations or measurements. I attempted the latter, but with no satisfactory result. The edges of the planet were soft with the requisite power, and their flickering at the webs of the micrometer defied my attempts to get a certain observation. I then tried the double-image micrometer with little better success: two observations of the polar diameter gave 19″·13, and two of the equatoreal gave 20″·56; both too large, I have no doubt, as might be expected from the diffused state of the limbs. These obs. give

$$e = 0{\cdot}06955 = \frac{1}{14{\cdot}38}.$$

"I shall try again under better circumstances, when such are to be had.

"Thanks for the communication of your idea respecting the faint ring. I could enter upon no such niceties on Saturday. But,

if you will refer to observations I long since made and published, you will find as strong a confirmation of your happy suggestion as you can desire, or are likely to receive from future observations, except under circumstances of rare occurrence. In the *Monthly Notices* for Jan. 1851, page 52, you will find I have said, 'I have always observed that the upper (southern) and more distant portion of the obscure ring is more plainly seen than the corresponding portion on the side nearest to the earth, and also that the projection of it at its minor (misprinted *inner*) axis is considerably narrower than accords with its breadth at the major axis.' In writing at the same time to the *Astron. Nachrich.* (see No. 751), after stating as above, I have added, 'To avoid the irradiation in diminishing its (the projection's) apparent breadth, I have carefully examined this dark line in daylight, and strong twilight, when the air was quiet and the image sharply defined. By comparison of its breadth with that of a spider's-web of the micrometer, I concluded it to be not more than from 0″·2 to 0″·25 at the minor axis, which, for the time of observation, corresponds to a breadth of from 1″·08 1″·35 at the major axis, instead of nearly 2″ as I had measured it. The appearances would be satisfied by supposing that a section of the obscure ring may be somewhat *wedge-shaped*, the thicker edge being outwards, and that a section of the bright ring may also be wedge-shaped, its thickest edge being inwards.

"But all this bears directly on your supposition that the obscure ring is not in the same plane as the others; and the observations are better satisfied by it than by any other hypothesis.* That the projection on the ball is not seen as broad as it ought to be I have always observed, and mentioned it in my last paper with reference to your diagrams of *Saturn*, in which the projection required to be *narrowed* to agree with the view my telescope gives me; and in the beginning of 1851 the difference appeared much greater than it does now, which would agree with your suggestion. I did not, indeed, notice that the *southern* part of the faint ring *encroached* on the *inner edge* of the *bright ring*. But in every other respect nothing could more fully support your hypothesis than the observations which I have uniformly made. Some other things strike me about it, which I have no time to mention now, being obliged to conclude hastily.

"Yours truly,
"W. R. Dawes."

"*Wateringbury, Nov.* 29, 1852."

Second Letter from Rev. W. R. Dawes to Mr. De La Rue.

"While I was writing to you yesterday, it occurred to me that in 1848, while the ring of *Saturn* was invisible, I had observed *two*

* Mr. De la Rue states, that this hypothesis is founded on his observations of the *encroachment* of the dark ring on the southern portion of the inner bright ring and its apparent *contraction* on the opposite side; also on the circumstance of the apparent ellipticity of the dark ring not being the same as that of the bright rings.

dark lines crossing the equatoreal region, one darker than the other, and separated by an exceedingly fine bright line. I had at that time explained the appearance by the supposition that the *lighter* of these two lines was the *projection of the rings* on the planet, and the *darker* their shadow. (See *Monthly Notices*, vol. x. p. 48, under dates Oct. 6, 26, 30; and Nov. 21; and Remark 3, at the end of my paper). It struck me as possible that your suggestion might afford a different explanation of the appearance, and thereby gain additional support to itself. On referring to my observations at that epoch, and considering the relative positions of the sun, the earth, and the planet, and also how the faint ring must have lain at the time with reference to the other rings on your hypothesis, I find that the explanation I gave at the time must have been the true one, and that the faint ring must have been *enveloped in the shadow* of the others, if its plane deviates as you suppose.

"I recollected also a figure of *Saturn* at the same epoch, by Julius Schmidt, of the Observatory of Bonn, in which are shown two similar dark lines to those I described as observed by myself. (See *Astron. Nachrich.*, No 650, and illustrative lithograph, fig. 17). But the same explanation applies to that which applies to my own; so that this support to your hypothesis fails. Nothing, however, arises from this *in opposition to it*; as, even supposing your idea to be correct, the faint ring must then have been simply invisible.

"From Mr. Johnson's measurements of the equatoreal and polar diameters I deduce

$$e = 0{\cdot}09940 = \frac{1}{10{\cdot}06}$$

as an approximate result, omitting the reduction of the polar diameter measured to the true semi-minor axis.

"In like manner, from your measures, we have

$$e = 0{\cdot}08139 = \frac{1}{12{\cdot}29}$$

"Both of these results are considerably less than that deduced by Mr. Main in 1848, viz.

$$e = 0{\cdot}10838 = \frac{1}{9{\cdot}227}$$

"I suspect that, without great caution, the polar diameter may be *squeezed too close* in the double-image micrometer and in the heliometer. The *brightness* of the poles is so much less than that of the equator, that, without special precautions, the measurements of the polar diameter will almost certainly be taken too small. Those with the P. W. micrometer are not quite free from the same danger, for the inflexion of light at the webs is much more obvious at the bright edges of the equator than at the dull polar limbs, and the *irradiation*, too, will be greater for the same reason. I, therefore, *greatly doubt* whether the ellipticity be anything like what Mr. Main deduced. Draw out such an ellipse, and I think you

will say you never saw *Saturn* of that figure. And the eye may sometimes be trusted rather than the micrometer. Recollect, too, that the ellipticity of *Jupiter* deduced from the Greenwich observations from 1840–1848, is little more than *half* what Mr. Mr. Main's observations assign to *Saturn*, viz. $\frac{1}{17\cdot9}$.

"*Wateringbury, Nov.* 30, 1852."

Sur les Dimensions de Saturne, par M. Otto Struve.

(*Extrait des Mém. Acad. Scien. St. Pétersbourg*, tom. v.)

In this memoir M. Struve has given the results of a limited series of micrometrical measures of *Saturn* and his rings, which he executed at Pulkowa with the great refractor in the autumn of the year 1851. He also examines some observations of preceding astronomers, tending to throw light upon the physical constitution of the rings.

The obscure ring was very distinctly visible on these occasions. M. Struve perceived that at the extremities of the *ansæ* it was divided into two parts by a dark line. He is of opinion that the division extends throughout the entire circuit of the ring. The gradation of light from the ancient bright rings to the obscure ring was very abrupt and striking; but he was unable to discover the slightest trace of a division between the exterior edge of the obscure ring and the interior edge of the inner bright ring. The breadth which he assigns to the obscure ring (2″·035), agrees almost exactly with that which Dr. Galle, of Berlin, obtained in 1838, and with a similar measurement of Mr. Dawes in 1851. (*Monthly Notices*, vol. xi. p. 52.)

M. Struve next proceeds to show that the observations of various astronomers anterior to the year 1838, afford unequivocal indications of the existence of the obscure ring. He remarks that the early observers generally make mention of a dark belt (termed the equatoreal belt) which was seen passing across the body of the planet *immediately contiguous* to the inner edge of the interior bright ring. J. Cassini showed in 1715, that from its slight curvature the equatoreal belt could not be situate close to the surface of the planet. The observations of Hadley, a few years later, would seem to establish almost beyond doubt the identity of the equatoreal belt with the obscure ring of modern astronomers. In 1723 he remarked that the dusky line which in 1720 he observed to accompany the inner edge of the ring across the disc, *continued close to the same, though the breadth of the ellipse had considerably increased since that time.* (*Phil. Trans.* 1723, No. 378.) He asserts, moreover, in reference to the hypothesis of the belt being produced by the shadow of the ring upon the planet, that when he considered the situation of the sun with respect to the ring and the planet, he found that the belt could not arise from such a cause.

As confirmatory of the same hypothesis, M. Struve remarks that the projection of the obscure ring upon the body of the planet agrees with the position assigned by the earlier observers to the equatoreal belt, and that no trace of the latter is visible upon the planet in the present day. He therefore arrives at the definitive conclusion that the obscure ring has existed around the planet at least since the time when telescopes were first constructed of sufficient optical power to exhibit the belts upon his disc.

The most serious objection which appears to M. Struve to offer itself to this hypothesis is founded on a remark made by Maraldi in 1715, to the effect that, during the disappearance of the ring in that year, the equatoreal belt continued still to be visible. If, indeed, that phenomenon be attributed to the projection of the obscure ring upon the body of the planet, it is difficult to imagine how it could have been perceptible on the occasion of the passage of the sun through the plane of the ring. M. Struve suggests as a probable explanation of this fact, that the obscure ring may be considerably thicker than the bright rings. The phenomenon, if sufficiently established, might perhaps serve to strengthen Mr. De la Rue's hypothesis of the obscure ring being situate in a different plane from that of the bright rings. It is so far confirmatory of the observation of Maraldi that M. Schmidt, of Bonn, perceived an obscure belt across the equator of the planet on the occasion of the passage of the sun through the plane of the ring in the year 1848 (*Ast. Nach.* No. 650). Future observations made during the disappearance of the rings can alone place the existence of the anomaly beyond all doubt.

The breadth of the space which separates the inner edge of the interior bright ring and the body of the planet, having exhibited a considerable discordance, as measured by himself in 1851, and by his father in 1826, M. Otto Struve was induced to institute a careful examination of the various measurements of preceding astronomers relative to the dimensions of the planet and the rings. The conclusion at which he arrived on this subject is somewhat curious. By a comparison of the micrometrical measures of Huyghens, Cassini, Bradley, Herschel, W. Struve, Encke and Galle, with the corresponding measures executed by himself, he found *that the inner edge of the interior bright ring is gradually approaching the body of the planet, while at the same time the total breadth of the two bright rings is constantly increasing.* It may be remarked, that the earlier measures employed in this comparison are founded upon certain assumptions relative to the effects of irradiation. They cannot, therefore, be regarded as altogether trustworthy.

In the concluding part of his memoir, M. Struve examines whether the two bright rings participate in an equal ratio in the enlargement of breadth which he found them to be constantly undergoing when considered as one appendage. He infers, from a comparison similar to that instituted in the previous case, *that, during the interval which elapsed between the observations of J. D. Cas-*

sini and those of Sir William Herschel, the breadth of the inner ring had increased in a more rapid ratio than that of the outer ring. The subsequent measures seem to indicate a reversal of this process.

The view of *Saturn*, given in the present Number, has been carefully drawn to scale and engraved by Mr. Warren De la Rue. The *principal* dimensions are taken from the *Nautical Almanac*, so far as they extended. Mr. De la Rue suggests that the data should include the major and minor semi-axes of the ball of the planet; the semi-axis minor of the dark ring; and the semi-axes of the divisions, according to the best authorities. His opinion is, that the large and fine telescopes which are now in use would enable a careful observer with a practised eye to fill up, and even to correct, the representation of micrometrical measures, and that, if the data for delineating *Jupiter* were given, his figure might probably be corrected by comparing the representation with the planet itself, seen under the most favourable circumstances.

Mr. De la Rue proposes to unite in one sketch the phenomena of *Saturn*, which have been seen by MM. Dawes, Lassell, and himself.

Professor Chevallier finds from a comparison between his ephemeris of *Hygeia* and observation that the errors alter very slowly; they are as follows :—

	Computed—Observed. R.A.	N.P.D.	
1852.		°	
Oct. 24	−30·54	+27·6	
Nov. 2	30·87	31·5	Cambridge Meridian
30	32·08	48·1	
Dec. 2	−31·96	+50·9	

He has, therefore, thought it better, " instead of attempting to correct the elements from partial observations, to compute accurately the planet's places from the same elements, during the entire period of its present appearance."

The ephemeris has been printed by the Professor, who has had the kindness to send copies for distribution to the Royal Astronomical Society.

BIELA'S COMET. *Ast. Nach.* No. 836, p. 327.

The following and southern portion of Biela's comet was observed at Cambridge on Sept. 8, 16, 21, 1852. Professor Challis finds that the Roman observations of August 25, 27, 28, and Sept. 1, as well as all the Cambridge observations, belong to the *southern* and *following* portion. The observation of Professor Secchi on Sept. 15, and the Berlin observations of Sept. 17, belong

to the *preceding* and *northern* part. Professor Challis further remarks, that the observations at Rome on Sept. 19 and 20, and at Berlin Sept. 22, confirm the above conclusions. Professor Secchi says, that the following head is the brighter of the two, and this probably corresponds to the larger head in the apparition of 1846.

The publication of *Cometic Orbits, with copious Notes and Addenda, compiled and edited by* Edward J. Cooper, 8vo. pp. 196, Dublin, 1852, was announced, but, unluckily, a copy for reference was not in the room. This work has already been alluded to by Mr. Graham.*

It is well known to the cultivators of cometic astronomy, that Dr. Galle appended a corrected list of all the computed cometary orbits which had been observed before June 1847, to Professor Encke's edition of Olbers' celebrated *Abhandlung*. But this work, notwithstanding its great accuracy and research, is in a considerable degree inaccessible to many English astronomers on account of its language; and the brevity of the notes implies a readier access to a large library than is usually the case. Mr. Cooper and his coadjutor, Mr. Graham, speak in the highest terms of Dr. Galle's labours, and have availed themselves of his researches: when they had not the means of consulting the reference, they have relied upon his authority in preference to any other.

Mr. Cooper's catalogue of comets extends to IV, 1851, which passed its perihelion Sept. 30·8, mean Greenwich time. The dates in the volume are *all* reduced to Greenwich time.

As to the authorities adopted for the construction of this catalogue, Mr. Cooper says:—"In the selection of orbits of any comet, where a great number have been computed, I had the advantage of Mr. Graham's assistance. Our adopted rules were to prefer those calculated upon the largest arc, having regard to the soundness of the data, and the reputation of the computer; to reject parabolic and hyberbolic elements, where no doubt or but little existed as to the ellipticity of the orbit, and to preserve those elements of any one comet that agreed best with one another." Following up a remark made by Sir J. Herschel, *Outlines of Astronomy*, p. 377, that "a plane of motion nearly coincident with the ecliptic and a periodical return are circumstances eminently favourable to direct revolution in the cometary, as they are decisive among the planetary orbits" (an idea which had, indeed, occurred some years ago to Mr. Cooper himself), Mr. Cooper "thought it desirable to investigate whether or not any other apparent analogy might be deducible from the heliocentric longitudes of perihelions and ascending nodes." To ascertain this, the comets having ecliptic orbits were arranged in tables as follows:—

* Vol. xii. p. 34.

Table I.—Ecliptic orbits in the order of their inclinations.
Table II.—Comets whose revolutions are under 500 years, arranged in quadrants of the longitude of perihelion, commencing with from longitude 45° to 135°, and in the order of their inclination in each quadrant.
Table III.—Planets arranged in quadrants, as in Table II.

From a consideration of these tables, Mr. Cooper is induced to believe that to Sir J. Herschel's hypothesis "that direct motion or a small inclination are favourable indications of a periodic comet," we may add "a longitude of the perihelion and of the ascending node between 45° and 135°."

The notes appended by Mr. Cooper are very full and very instructive, and intelligible: they form, indeed, an essential feature in the volume.

Astronomische Beobachtungen auf der Sternwarte zu Bonn, *aufgestellt und herausgegeben von* Dr. F. W. A. Argelander. *Zweiter Band,** 1^te^ *Abtheilung.* 4to. pp. 224. Bonn, 1852.

This work is a continuation of the zone observations of Professor Argelander, from Zone 205 to 321, and contains stars between 15° and 31° of south declination. It may thus be considered as a prolongation of Bessel's Zones to the south, as the previous volume was an extension to the north. There are auxiliary tables, after Bessel's method, for the reduction of these observed places to the mean places for 1850, Jan. 0; but as the stars in these zones are observed so near the horizon, Professor Argelander has added a peculiar set of auxiliary tables as an appendix to the zones, by which the labour of reduction is considerably reduced.† Professor Argelander proposes to bring out these Southern Zones in three parts (*Monthly Notices*, vol. xii. p. 25), and hopes that they will give additional accuracy to past and future planetary and cometary observations made out of the meridian.

Professor Argelander remarks that the declinations of the southern stars in the B. A. Catalogue appear to be a mean of Taylor and Brisbane, and that Brisbane's catalogue sets the stars considerably too much to the south. It is much to be regretted that Mr. Taylor's second catalogue (*Madras Observations*, vol. vi., Madras, 1844), appeared *after* the completion of the B. A. Catalogue. Mr. Baily had only Taylor's *partial* catalogues to refer to, which are affected by a systematic error in the division of the

* The first volume, Bonn. 1846, contains the zones up to 205, which include the stars between 45° and 80° of north declination. Bessel's zones from 15° south declination to 45° north declination are to be found in the volumes of the Königsberg Observations. Of the latter, the stars between 15° north and 15° south declination have been reduced and catalogued. *Catalogus Stellarum ex Zonis Regiomontanis, auctore* M. Weisse, 4to. pp. 284. Petropoli, 1846.

† *Hülfstafeln zu den Bonner südlichen Zonen*, 12mo. pp. 99. Bonn, 1852.

Madras circle. In Mr. Taylor's second catalogue this error is corrected.*

Annalen der K. K. Sternwarte in Wien, dritter Folge, erster und zweiter Band. 8vo. Wien, 1851–2.

The particular title of this book is, *Argelander's Zonen-Beobachtungen vom* 45° *bis* 80° *nördlicher Declination in mittleren Positionen für* 1842·0, *geordnet,* von Wilhelm Oeltzen.

This reduction of Argelander's northern zones was in part performed by M. Oeltzen before the autumn of 1850, when he was attached to the Vienna Observatory. Its early completion is due to M. Littrow, the director of the Observatory, who relieved M. Oeltzen as much as possible from his ordinary work.

The catalogue contains 26,425 stars, arranged in order of right ascension, with the magnitudes, right ascension to hundredths of a second, and declination to tenths of a second. To each star is appended the zone and number which belong to it in Argelander's observations. There are no precessions given, but two tables by which they may be computed, and the reduction to another epoch readily effected. A short introduction of twenty pages (in German) explains M. Oeltzen's mode of procedure.

Logarithmorum VI. Decimalium Nova Tabula Berolinensis, &c. Auctore Carolo Bremiker. 8vo. Berolini, 1852.

This volume contains the logarithms of numbers and logarithmic sines, cosines, and tangents for every 10″ to six places of decimals. There is an introduction (82 pages) in Latin, explaining the use and construction of the tables. The work is very clearly printed, nicely arranged, and of a convenient size.

Elements and Ephemeris of Westphal's Comet (II. 1852), by August Sonntag.

The periodic time is 60·8 years. The ephemeris extends from 1852, Nov. 12, to 1853, Feb. 1. *Ast. Nach* No. 836, p. 322.

Elements of Psyche, compared with all the observations which could be collected, by M. Klinkerfues, of Göttingen Observatory. *Ast. Nach.* No. 831, p. 234.

* This valuable Catalogue is also published as an independent work: "A General Catalogue of the principal Fixed Stars [11,015], from Observations made at the Honourable the East India Company's Observatory at Madras in the years 1830–1843, by Thomas Glanville Taylor, &c." 4to. pp. 272. Madras, 1844.

Comparison by M. Brorsen of all the Observations of the Comet II. 1851, with his elliptic elements: published in No. 782, *Ast. Nach.* No. 827, p. 270.

Notice of the establishment of an observatory at Toulouse, and a particular account of a shooting star observed at Bourdeaux and Toulouse, with the elements of its trajectory, by M. F. Petit, Director of the Observatory of Toulouse. *Ast. Nach.* No. 834, p. 287.

Observations of Shooting Stars on the evening of the 9.10th of August, 1852, on board the Ville de Paris. By M. E. de Jonquières. (Letter to M. Arago.)

(*Comptes Rendus, Sept.* 13, 1852.)

These observations were made on the eastern coast of the island of Sardinia. The number of stars which appeared every hour amounted to about seventy. The purity of the atmosphere allowed the trace of each *fusée* to continue sufficiently long to be enabled to form a good estimate of its direction. All the stars appeared to diverge from the same point of the heavens. This point was situate between 35 (*Hev*) *Cassiop.* and 33 α *Persei*, but somewhat nearer the former than the latter of these two stars.

Note on the Means of diminishing the Oscillations produced at the Surface of Mercury in the vicinity of ordinary Roads, Railroads, and Manufactories, with the view of facilitating Astronomical Observations. By MM. Séguin and Mauvais.

(*Comptes Rendus, Oct.* 11, 1852.)

The experiments were made at Fontenay. No sensible amelioration was obtained by placing the vessel containing the mercury upon elastic bodies, such as bales composed of layers of caoutchouc piled above each other, empty balloons of caoutchouc, &c. &c. When the vessel, however, was *suspended* through the intervention of an elastic substance, the oscillations were in every instance diminished. When vulcanised caoutchouc was used for this purpose the oscillations were completely annihilated, it having been impossible to perceive the slightest trace of them when the reflected images of different objects were observed with a portable telescope of moderate magnifying power. In this case the vessel was suspended by a cord passing through the caoutchouc disposed in the form of a ring.

On the Periodic Return of the Solar Spots.

(*Comptes Rendus, Nov.* 8, 1852.)

M. Wolf, Director of the Observatory of Berne, mentions, in a letter to M. Arago, that he has recently been engaged in researches on the solar spots, and has arrived at some interesting conclusions on the subject. By a comparison of all the observations of the spots made from the epoch of their discovery down to the present time, he has discovered that the number visible upon the surface of the sun in the course of a year recurs at regular intervals of time. The mean duration of the period comprised between two maxima or minima he finds to be $11{\cdot}111 \pm 0{\cdot}038$ years, which, he says, agrees much better with the variations in declination of the magnetic needle than the corresponding period of $10\frac{1}{3}$ years assigned by M. Lamont. He has also ascertained, that the years during which the spots have been most numerous, have been also the driest and most fertile, agreeably to a remark of Sir William Herschel's.

Note sur la Disposition la plus favorable à donner aux Appareils destinées à atténuer les Vibrations de la Surface du Mercure, et sur les Moyens d'approprier les Appareils à l'Usage des Instrumens méridiens. Par M. Mauvais.

(*Comptes Rendus, Nov.* 15, 1852.)

M. Mauvais had previously communicated the details of a series of experiments, by which it was found that the oscillations of the mercury were almost completely extinguished when the vessel containing it was suspended by an elastic substance composed of vulcanised caoutchouc. In the present note he proposes the following mode of applying this principle to meridional observations.

In a horizontal plank four vertical bars are inserted, and to their extremities are attached an equal number of straps of caoutchouc, having their lower ends fixed in a circular horizontal plate destined to receive the vessel containing the mercury. In order to maintain the circular plate in a steady position, a rod is attached underneath at the centre of the plate, carrying at its extremity a heavy ball.

Note de M. Faye sur les dernières Resultats publiées par M. De Struve relativement à la 61^{e} du Cygne.

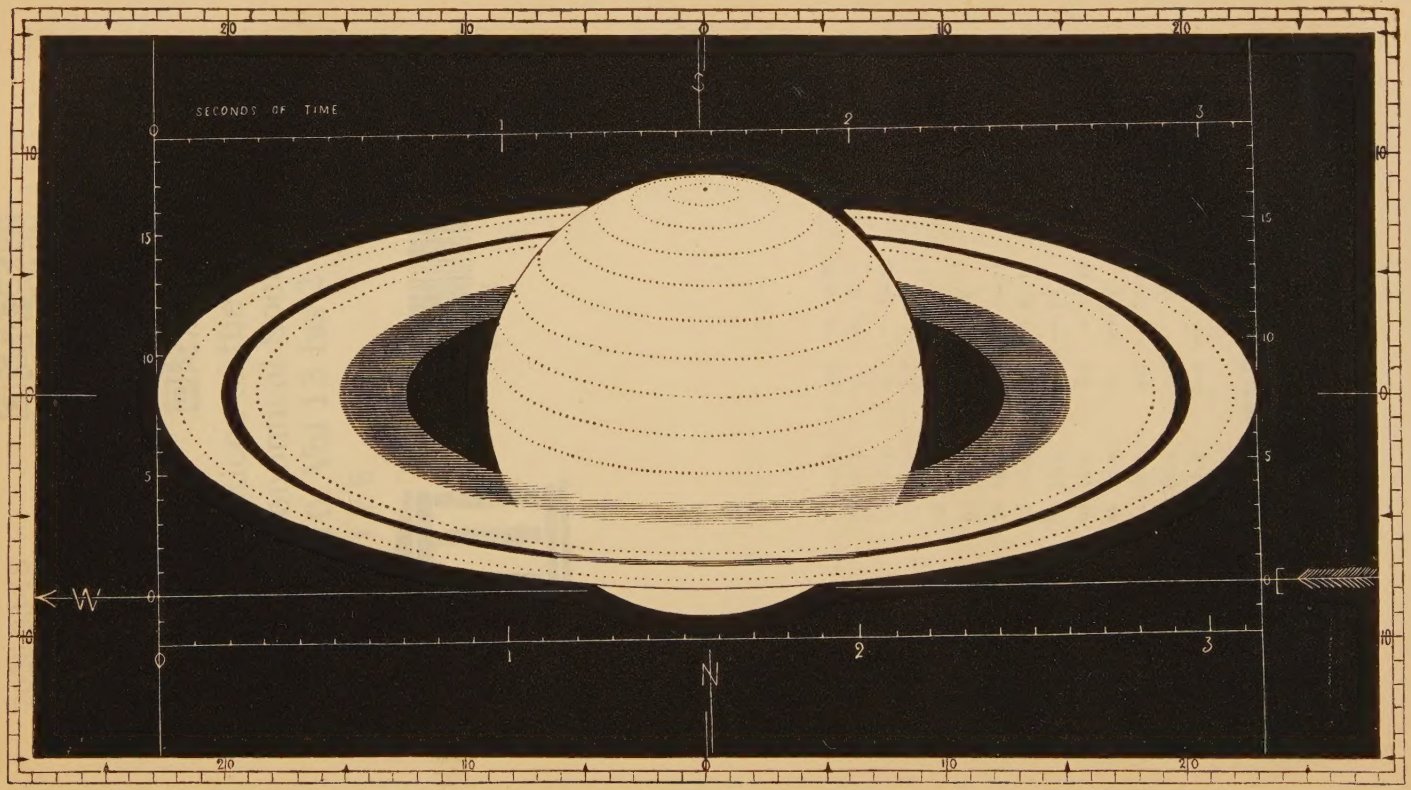
(*Comptes Rendus, Nov.* 22, 1852.)

In this note M. Faye calls attention to the fact that M. Struve, in his recently-published Dorpat Catalogue, has announced that, during the last hundred years, the relative motion of the components of this star has been sensibly uniform and rectilinear. This result essentially vitiates all the calculations that have been hitherto made respecting the orbit and mass of the star.

The instruments of the late Dr. Pearson are at present on sale; they may be viewed at Mr. Ross's, 2 Featherstone Buildings, Holborn. Among many valuable articles, the *Azimuth and Altitude instrument*, designed and partly constructed by Troughton, but finished and divided originally by Thomas Jones, and the *Astronomical Telescope*, $6\frac{1}{2}$ inches, ground by Tully, from glass presented by Guignand to the Astronomical Society, may be mentioned as remarkable instruments.

London:—Printed by G. Barclay, Castle St. Leicester Sq.

Seconds of Arc.



November 16th, 1852.

WARREN DE LA RUE, DEL.